Handy Atlas of Modern Geography. (London: Edward Stanford, 1892.)

IT would be difficult to obtain a small atlas more complete than this. Every place of any importance appears to be represented on one or more of the thirty coloured maps. The degrees of latitude and longitude are subdivided into parts of five minutes each, so that the positions of places, the names of which are not engraved, can be easily and accurately located by reference to the alphabetical list at the end. This list is a comprehensive one. It gives the latitude and longitude of the principal mountains, rivers, capes, bays, islands, towns, and villages, and forms an excellent supplement to a very good atlas.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Aurora.

A VERY brilliant display of aurora was seen here last night, the 25th inst. At about 9.25 p.m. a number of red streamers proceeded from a length of some 110° in azimuth along the northern horizon, and extended upwards for (on an average) 30°. The length of the streamers varied quickly, sometimes shooting upwards for 70° from the horizon. In the course of five minutes the red streamers gave way to white or yellowish white ones, narrower and more sharply defined than the red ones. At 9.40 p.m. there was a decrease in the brilliancy of the phenomenon, but at 9.45 p.m. long red streamers again appeared for a few minutes, which again shortly gave way to a brightness of the horizon only. Close to the horizon the colour was white, or nearly so, the whole time. The apparent point of convergence of the streamers was far south of the zenith, say 30°.

GEO. M. SEABROKE.

Temple Observatory, Rugby, April 26.

PROBABLY many of your readers witnessed the brilliant display of the northern lights between nine and twelve o'clock last night, the 25th, but it may be as well to call attention to it, as being the finest display seen here for many years. Appearing soon after nine o'clock, the luminous arc and the radiating beams, sometimes rose and orange coloured, presented a varied and beautiful spectacle until close upon midnight, when they faded away.

The most noteworthy features of this display were the vividness and height of the arc, which reached an angle of about 13° above the horizon, whilst the beams were visible up to 51°. The whole expanse of the arc from east to west was about 93°, and the duration of the phenomenon a little under three hours.

ARTHUR MARSHALL.

Cauldon Place, Long Row, Nottingham.

A FAIRLY distinct aurora was visible here on the northern horizon last night. I first observed it at 9.15, when the streamers appeared somewhat less bright than the Milky Way. Ten minutes later one streamer, about 15° west of north, brightened considerably, and appeared of a pale reddish-yellow tint. It fluctuated in intensity, and soon became less bright. The streamers, which inclined slightly to the west of the vertical, extended to about 30° to 40° above the horizon.

I watched them till 9.50, when they seemed fading in intensity, and when I looked again at 10.30 they had disappeared entirely.

ARTHUR E. BROWN.

Thought Cot, Brentwood, April 26.

Pigments of Lepidoptera.

THE appearance of Mr. F. Gowland Hopkins's letter on this subject in the last issue of NATURE (p. 581) demands a brief explanation from me—although it is not easy to reply satisfactorily within narrow limits—and the more so since Mr. Hopkins appears to have somewhat misunderstood my standpoint.

Let me first acknowledge the courteous tone of Mr. Hopkins's letter, and express my sense of the value to myself of criticism from his pen, and the more so since I have been labouring under the disadvantage of being practically entirely uncriticized so far—a disadvantage that I have not failed to appreciate.

Now, Mr. Hopkins remarks: "Mr. Coste's experiments are very useful as forming a method of classifying these pigments; but . . . they are of far too empirical a nature for any considerations as to the constitution of the bodies to be based upon them."

Here it is that Mr. Hopkins appears to have missed the point of my work. If he will do me the favour to refer to the detailed account of my experiments in the *Entomologist, passim*, I think that he will find it tolerably clearly emphasized that my interest in this work, so far, has been almost entirely *biological*. I stated expressly in my opening article that my object had been to discover, if possible, the genealogies of the colours, and to obtain evidence (so far as coloric characters could afford it) of the phylogenetic relations of allied species: and I may perhaps add that the results obtained have enabled me to predict several varieties of whose occurrence in the natural state I have since been informed. So that Mr. Hopkins is mildly reproaching me because my work does not tend in a direction at which it was not originally aimed, while he is at the same time good enough to admit that it is of some use for the end at which it was aimed.

However, it was only to be anticipated that one could not go very far without becoming involved in the further question as to the constitution of the pigments; but here I was met by three considerations. In the first place, I was anxious to obtain first of all as much as possible of what Mr. Hopkins designates "empirical" evidence as to the reactions and classification of the pigments before making any researches at all into their constitution; secondly, the amount of material at my command was far too scanty for any even approximate analysis; and in the third place, shortly after my experiments had been commenced, my attention was drawn to an abstract of a paper by Mr. Hopkins on the constitution of the yellow pigments. Finding that he was already in possession of the field here, I felt almost bound to leave this part of the subject alone, at least for the present; and I think that I may say that I have on the whole taken exaggerated care not to extend my experiments into that quarter where Mr. Hopkins was engaged, or to avail myself of the discoveries that he had already made, in order to trespass on his investigations. Putting aside my provisional suggestions as to the nature of the "reversion effect," it has only been at a comparatively recent stage of my work, and in consequence of experiments that have not yet been published, that I have at all turned my attention to the constitution of the pigments; these results being such as would have compelled me to consider the question even had I heard nothing of Mr. Hopkins's work. I hope that this explanation will put me right in Mr. Hopkins's eyes, and will satisfy him that he has considerably misunderstood the spirit of "some remarks [perhaps clumsily expressed by me] made at the close of the last article"; and that it will also satisfy him as to the question of priority. I had no thought of questioning Mr. Hopkins's priority in his own work, and the less so since I have throughout been under the impression that we were working mainly on different—though sometimes adjacent—lines.

I must not so far trespass upon your space as to criticize Mr. Hopkins's criticisms upon the "reversion effect"; but I will ask him kindly to examine the detailed accounts of the "reversion" experiments which I gave in the *Entomologist*, since his remarks appear to me somewhat to ignore the evidence there brought forward: and at the same time I may remark that his statements as to the constitution of the *yellow* pigments appear to me hardly to invalidate, but rather indirectly to confirm, the suggestions made by me as to the reversion reaction with *red* pigments. The new information that Mr. Hopkins promises in his closing paragraph I shall look forward to with great interest.

April 22. F. H. PERRY COSTE.

I WAS about to pen some remarks on Mr. Perry Coste's recent articles on this subject, when a letter from Mr. Gowland Hopkins in the last number of NATURE (p. 581) expressed substantially the same views as those which I had arrived at. I write now rather to support Mr. Hopkins in his strictures than to offer any fresh criticisms of my own. The articles on "Insect Colours" published in these columns are, as the author

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states, to be regarded in the light of an abstract of a series of more extended papers published in the *Entomologist.* The papers in the latter publication from their title led us to suppose that Mr. Coste had made some contribution to our knowledge of the chemistry of insect pigments. I read them from month to month in the hope of getting new light on this subject, which is of such general interest to both chemists and biologists: I regret to say that I have been grievously disappointed. The experiments thus far described amount simply to the fact—not altogether astonishing—that strong chemical reagents modify the colours of Lepidopterous pigments or in some cases dissolve them out of the wings. The bearing of these observations on the chemistry of the pigments is so remote as to be practically useless until we know something of the chemical nature of these pigments. The methods adopted by Mr. Coste are not likely to advance our knowledge in this direction very much, and it is certainly remarkable that in treating of yellows he makes no reference[1] to the only real contribution to the chemistry of Lepidopterous pigments, viz. the experiments made by Mr. Hopkins, and published in the Proceedings of the Chemical Society in 1889. Mr. Coste is no doubt acquainted with those South American *Papilios* with a large red spot on the hind wing, which spot loses its red colour and becomes of a brilliant metallic bluish green when the wing is tilted so that the incident and reflected rays form a very wide angle. The colour is in this case doubtless a mixed result, partly due to pigment and partly to interference. Now, anyone who has observed this and other similar colour phenomena in insects might describe his observations as contributions to the physics of insect colours. If he thought proper to adopt this course, he would be misleading physicists. The observation of the bare facts is as much a contribution to the physics of insect colours as the statement that a rainbow can be seen in the sky is a contribution to the physics of illuminated water-drops. It seems to me that Mr. Coste's experiments bear the same relationship to the chemistry of insect colours that the mere observation of interference colours in insects bears to the physics of insect colours.

Quite independent of the facts recorded by Mr. Coste is the interpretation which he puts upon them. Here I must decidedly express dissent. It cannot be admitted, because by the action of certain reagents green is changed into yellow or red into yellow, that this indicates the evolution of green or red from yellow. There is no evidence that this result is a reversion effect at all. The analogy between the action of strong acids in modifying the colour of an animal pigment and the effect of true reversion is forced, and has no parallel in natural processes. Hot water is a chemical reagent; by its action on the brown pigment of the lobster the latter becomes red. If from this observation I drew the inference that the ancestral lobster was red, and that the hot water produced a reversion effect, I do not think that Mr. Coste would agree with me.

R. Meldola.

Oxford, April 24.

Eozoon.

Mr. Gregory has, I fear, slightly mistaken the meaning of my remarks, which were intended rather to excuse than to blame him. The specimen of Eozoon collected by the late Mr. Vennor at Tudor was figured in connection with my paper of 1867 as a type specimen, in so far as macroscopical characters are concerned; but it does not follow that slices from specimens less perfect in that respect, and now in my collection, may not be more instructive as showing minute structures. I may refer in this connection to the three specimens from Tudor and Madoc (Madoc being in the same formation with Tudor) figured by Dr. Carpenter in our original paper in the Journal of the Geological Society, vol. xxiii., pl. xii., Fig. 1. If anyone will take the trouble to compare these with the figures in Mr. Gregory's paper in the same Journal, vol. xvii., he will have a singular and impressive illustration of the different ways in which things supposed to be the same may appear to observers of different types.

Mr. Gregory is in error in supposing that he could see in the cases of the Peter Redpath Museum my specimens from Tudor and Madoc. I have not yet been able to place there any portion of my microscopic cabinet of Eozoon; but only a few hand specimens sufficient to show students the ordinary types of the fossil.

As to the Laurentian age attributed to the Tudor beds, I have already explained that this I subsequently regarded as an error, and so stated not long after the date of the paper of 1867. I now regard them as less ancient, though of pre-Cambrian age.

I shall be happy to show to anyone my little collection from Tudor and Madoc, including specimens in which Carpenter detected the canal system; but of these particular specimens I have unfortunately no duplicates for distribution, and would prefer to exhibit the slices in the modes I have found best suited for the development of the structures; as otherwise there might be some doubt whether the resulting impressions would more resemble Mr. Gregory's figures or Dr. Carpenter's.

Montreal, April 6. J. William Dawson.

The Theory of Solutions.

I am glad to see that as to the main point, the character of the "gaseous laws" of solutions, there seem to exist no more differences between Mr. Rodger and me. For Mr. Rodger, in his letter on p. 487 of Nature, limits his remarks to some dialectical expressions, to cover an honourable retreat. I wish not to follow him on this way, because it is an endless one.

As to the application of van der Waals's formula on solutions, Mr. Rodger is now forced to confess that this application is not so "meaningless" as he has formerly written; but he asserts that, shortly spoken, the form of application given in my book is so. To say the truth, if I have to choose, as in this case, between the agreement of a formula with Mr. Rodger's opinion, and the agreement of this same formula with experiment, I prefer the latter.

W. Ostwald.

Leipzig, April 12.

Physiological Action of Diminished Atmospheric Pressure.

With reference to the effect of diminished atmospheric pressure on the vital powers, alluded to in Prof. Bonney's review of Mr. Whymper's "Travels among the Great Andes of the Equator" (Nature, April 14, p. 561), I do not know whether it is worth while recalling the well-known fact that numerous passes in the Himalayas, ranging from 17,000 to 19,000 feet, are habitually traversed by the hillmen, in the summer, with their flocks of sheep and goats carrying borax, &c. The highest pass is said to exceed 20,400 feet. In the same mountains Messrs. Schlagintweit reached an altitude of about 22,200 feet (Proc. As. Soc. Bengal, January 1866), while Mr. W. W. Graham ascended to 23,500 feet in 1883 (Nature, September 11, 1884). I have myself, on several occasions, been to elevations of 17,000 to 19,000 feet, and beyond shortness of breath when climbing, never experienced any ill effects except once, when I, the four plainsmen with me, and three out of a considerable number of hillmen, felt severe headache during the evening after crossing a high pass. My companion on one trip, however, almost invariably suffered very severely from mountain sickness under similar circumstances.

F. R. Mallet.

18, The Common, Ealing.

Sensitive Water Jets.

A form of this effect lately presented itself, which seemed in some ways new. A thin jet, 5 feet high and arched so as to be 3 feet at the base, was falling in a feathery spray. At 13 feet distance a small Wimshurst machine was set going: not instantly, but after two minutes, the spray gathered itself up almost into one clear line: although the jet was turned up and down and the machine was discharged the falling water would not resolve itself again into spray for fifteen or twenty minutes. It is difficult to imagine the medium for this action: it is too indefinite, perhaps, to suppose that an indicator is found for the trembling of a disturbed ether while it is dying down.

The well-known experiment is not known enough, for it is not often described in books. Take a glass rod, electrified ever so little, to a certain point; at once the jet collects itself; a slight move away brings back the old disorder, while an inch nearer makes things much worse. It is a striking illustration to help one to imagine what the electrical forces of the air may do We can perhaps understand those thick thundery rain-drops, that almost allow us to pass between them while they are giving friendly warning of what will come.

W. B. Croft.

Winchester College, April 14.

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[1] At least in Nature: I have not the *Entomologist* at hand where I am writing.

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